LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Eozoön Canadense

I CANNOT understand Mr. T. Mellard Reade's right to fling the taunt at those who maintain the foraminiferal nature of Eozoön, that "each disputant takes up a different position, and shifts it as occasion requires."

I have never taken up any other position than this: that *the best-preserved specimens of the Canadian Eozoön exhibit an unquestionably foraminiferal structure.* I am supported in this by every British naturalist with whom I am acquainted, as specially conversant with foraminiferal organisation, viz., by Messrs. H. B. Brady, T. Rupert Jones, W. K. Parker, and Prof. W. C. Williamson; whilst the most eminent authorities in micro-mineralogy and pseudomorphic structure, viz., Messrs. David Forbes, T. Sterry Hunt, and H. Sorby, altogether disown Eozoön as a mineral.

I have further asserted, and I do not in the least "shift" my position, that *the character of the Canadian Eozoön is altogether independent of that of later ophites.* The occurrence of true Eozoic structure in the newest Tertiaries would only show that Eozoön, like Lingula, has maintained its continuity through a long succession of geological epochs. On the other hand, the occurrence of minerals presenting superficial resemblances to true Eozoönal structure, can be of no account to such as really understand the latter.

If the Skye ophite, for example, possesses a true "nummuline layer" in combination with other characteristic Eozoönal features, its presence in a formation of later "geological time than the Laurentian," furnishes no argument whatever against its organic character.

If, on the other hand, the supposed "nummuline layer" in the Skye ophite is nothing but a lamella of chrysotile, the existence of such a pseudomorph can only affect the opinions of such as are incompetent to distinguish the two by those microscopic tests on which experienced observers feel perfect reliance.

Since I do not feel called upon to expend valuable time in giving to Mr. T. Mellard Reade the instruction which he requires to qualify him for discussing this question, I now leave him to the enjoyment of his own opinion. Whenever he shall have shown, by work of his own, his competence to criticise the observations of others who have made a special study of the subject he discussed, I shall be most happy to afford him the same opportunity of forming his judgment as to the organic nature of Eozoön, by an examination of my preparations, that I have given to the many eminent naturalists, who have thus fully satisfied themselves of the justice of my conclusions.

W. B. CARPENTER

Dr. John Hopkinson on "The Overthrow of the Science of Electro-Dynamics"

As I see you have reprinted at length Dr. Hopkinson's paper with the above title, in which he criticises severely, not to say ungenerously, some papers of mine published in the *Quarterly Journal of Science* and *Chemical News*, you will think it only fair to publish my reply, in which I think I shall show that, in the course of his short paper, Dr. Hopkinson has committed mistakes at least as grave and important as any he imputes to me. Let us see if this is not the case.

Dr. Hopkinson quotes one of my articles as follows: "They (that is Joule and Scoresby) calculate the maximum theoretical power of a grain of zinc to be 158 foot-pounds, and yet using permanent magnets, which, by their own statement, were so badly constructed as to have only a quarter the power they ought to have had, with the poles of the electromagnets never approaching the permanent magnets nearer than $\frac{1}{4}$ of an inch (and what an enormous loss is incurred here!); with an engine constructed almost at haphazard, and with scarcely a consideration of the best principles or of the most advantageous construction of such engines, they actually obtained a result of 102·9 foot-pounds out of a calculated theoretical maximum of 158. With a little care and consideration, I do not hesitate to say the duty per grain of zinc might easily have been increased tenfold." On which he observes, "It is hardly credible, but the above looks very like a confusion between Force and Work! The author seems to assume that if the forces in operation in an engine are greater, that the engine will necessarily produce more work from the same quantity of fuel. In these experiments the quantity of zinc $(a-b)$ used to produce work W is observed; if the engine was made more powerful, if the permanent magnets were four times as strong, and the electromagnets passed $\frac{1}{8}$ of an inch from them, doubtless W would be greater, but so also would $(a-b)$, and it does not follow that $\frac{W}{(a-b)}$, with which we are concerned, would be at all changed. What becomes, then, of the dogmatic assertion that the duty or a grain of zinc would be increased tenfold?"

Why he should say, "It is hardly credible, but the above looks very like a confusion between Force and Work," I know not. I cannot plead guilty to having made the slightest confusion between the two. I do think the total of the force used is *a measure* of the work produced. But Dr. Hopkinson tries to persuade us that a well-constructed engine would do no more duty than an ill-constructed one, and consequently, I presume, that the magnets might possibly be weakened *ad infinitum*, and removed to ever so great a distance, without necessarily affecting the efficiency of the engine. And then he ventures to criticise my papers as full of fallacies! I retort that it is hardly credible, but that the above looks very like a confusion between $(a-b)$ and b! In these experiments of Joule and Scoresby's the quantity of zinc used to produce work W, is represented by the authors not as $(a-b)$ but as b, and therefore the duty per grain of zinc is not $\frac{W}{a-b}$ but $\frac{W}{b}$; and when the permanent magnets are stronger, and the electromagnets are passed nearer to them, not only does W increase but b also diminishes. So that was I not justified in saying that the duty of a grain of zinc could in a better-constructed engine be probably increased tenfold? And if it be increased only twofold, or even half as much again, then, allowing for waste, I have proved my point, and disproved Joule's mechanical equivalent of heat. Might I not retort fairly on Dr. Hopkinson that the Manchester Literary and Philosophical Society "never ought to have permitted this paper to appear in their Proceedings?"

Next let us take Dr. Hopkinson's next criticism. My argument is this:—That if the doctrine of the mechanical equivalence of heat be, *that production of energy absorbs, and destruction of energy produces, a definite amount of heat*, and if we find cases, as those of elastic wires, and water below its maximum density, in which destruction of energy produces cold, not heat, the doctrine of the mechanical equivalent of heat cannot be universally true. To this argument Dr. Hopkinson replies that the facts I quote as paradoxes are simple deductions from the two laws of thermo-dynamics. Quite true; but this only shows that one of the laws of thermo-dynamics is inconsistent with the doctrine of the mechanical equivalence of heat. Might I not retort again on Dr. Hopkinson that "a hostile critic should at least understand the meaning of what he criticises"?

If I said anything which *seemed* to imply that a minimum of work in an engine was inconsistent with a maximum of duty, I freely retract the expression; and I also acknowledge that the argument drawn from the fire-syringe had better have been omitted. But my point was proved abundantly without it.

But still, as the maximum of work done by a battery before it is worn out is only a multiple of the maximum duty of a grain of zinc, I do think it is a *startling* thing, though not mathematically impossible, that this maximum of work should prove to be no work at all.

Perhaps you will allow me to add that I have read Sir W. Thomson's paper read before the British Association in 1852, to which your own reviewer referred me. No doubt you will think it presumptuous in me to say so, but I think that in that paper he has mixed up two totally distinct questions, namely, the cold produced by the decomposition of water into its elements at two electrodes, and the heat produced by the resistance of the film of hydrogen or oxygen or oxide, to the passage of the current. The first is a fixed determinate quantity; the second an accidental one depending on the character of the surface of the electrode, and the ease with which it throws off the film of hydrogen or oxygen. These two points affect the question, as well as the polarisation, and the specific power of retaining or transmitting heat exercised by various electro-motive combinations. M. Favre suggests the formation, sometimes, of peroxide of hydrogen; but this supposition is unnecessary, and, moreover, would not remove the difficulty, for peroxide of hydrogen is so unstable a compound, that it would soon be resolved into oxygen

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and water, and thus exactly undo the thermal effect of its formation. I trust that in this letter, at least, you will find nothing unfit to be published in a scientific journal.

H. HIGHTON

P.S. I hope to discuss the question *vivâ voce* at the meeting of the Manchester Literary and Philosophical Society next week.

MR. HIGHTON has very effectively shown the uselessness (to him) of my review of his speculations. I wish I could withdraw it, and allow his letter to speak for him without any comments of mine. When a man can make the remarks he has made on Sir W. Thomson's paper of 1854, his case is hopeless—he is incapable of being taught even by the "grand founders" of the science. *Requiescat.* YOUR REVIEWER

Quinary Music

HAVING hitherto been under the impression that all the varieties of time in music might be regarded as made up of groups of two or three, and never having seen a single piece of music which in any way contradicted this view, Mr. J. Mullen's letter on "Quinary Music" in NATURE for the 9th inst. surprised me very much indeed. It is true that sometimes we meet with passages in which five notes are to be played in the time of four, or seven in six, &c.; but a piece of music in five time is a thing I at least have yet to see, and I should feel obliged to Mr. Mullen it he will kindly tell me the name and composer's name of this novelty. BEACON LOUGH

The Experimental and Natural Sciences in Trinity College, Dublin

I TRUST you will allow me to make a few remarks upon the article signed "W." which appeared in your last week's number.

In the first place, there are one or two inaccuracies which may be corrected. Thus, your correspondent states that a student "in his third and fourth years *must* devote himself, to a certain extent, to the study of Experimental Physics, including heat, electricity, magnetism, and chemistry, and pass examinations on these subjects, even at the ordinary term examinations." The sophister students are indeed *allowed* by the Board to substitute such a course for classics, but, it may be added, comparatively very few avail themselves of this permission. Again, it is declared that "the chemical and physical laboratories leave nothing to be desired." No such thing as a "physical laboratory" exists in the University; and, seeing that it is only lately that such things have been introduced at the other side of the Channel, it is scarcely likely that we shall have one here for some years to come.

But what I think especially calls for criticism is the general tone of the article, leading the reader, as it appears to me, to suppose that the Experimental and Natural Sciences occupy a high status in Trinity College, Dublin. Now this is far from being the case; there can be no doubt that they are still generally looked upon in the University as subjects of quite secondary importance. A.

Dublin, March 14

Science in Schools

I SHOULD be greatly obliged if you would kindly let me know of any school adapted for young boys whose parents wish to give them an education embracing the physical sciences and modern languages, on some such plan as that of the Realschule of Germany. W.

Dr. Donkin's Natural History of the Diatomaceæ

THE remarks of "K." on my review of the above-named work (*vide* NATURE, vol. iii. p. 348) fall under five heads. Allow me to briefly notice each. 1. There is a difference of opinion between us as to the execution of the plates in Part 1 of Dr. Donkin's work; my copy came direct from the publishing office, it is therefore presumably a fair specimen. I have once more examined the plates, as well as shown them to several competent judges, and the unanimous verdict of all is that expressed in my notice, *i.e.* the execution of the plates in this part is disappointing. I gave instances of apparent inaccuracy in detail in some of the figures. "K." passes these by, but cites figures that are accurate; surely there is nothing contradictory in this. My criticism related for the most part to the *execution* of the plates, and I confess that even comparing them to those illustrating Dr. Donkin's or Mr. O'Meara's papers on Diatoms in some of the late volumes of the *Quart. Journ. Micr. Sci.*, I would prefer the lithographic plates to the engraved ones.

2. "K." agrees with me about the synonymy. I agree with him as to the difficulty of this portion of the subject, but what value would this or any work on species have without synonyms?

3. "K." says the desirability of giving habitats in full is questionable, "three or four localities are sufficient." Unquestionably it is not desirable to give more than three or four localities for common forms, but I think it is equally unquestionable that when Dr. Donkin could have given as many localities for interesting and not extremely common forms, he was wrong not to have done so; and I alluded to the absence of Irish localities in the hope of removing an evident defect in an otherwise useful work.

4. "K." is, without doubt, right in referring the species given by him to Gregory, and not to Ehrenberg. I cannot imagine how so great a blunder on my part originated.

5. "K." asks, "who is Cleeve?" Cleve (not Cleeve), next to Heiberg of Copenhagen, is one of the best northern investigators of the lower Algæ. His monograph of the Swedish species of the Zygnemaceæ is well known, and in addition to his papers on Desmideæ and Oedogonium, he has published "Diatomaceer fran Spetsbergen" (1867) and "Svenska och Norska Diatomaceer" (1868). He naturally follows the arrangement of the "Conspect. Crit. Diatom. Danicarum." It is not without interest to observe how well acquainted these botanists are with the literature in our language relating to the Diatomaceæ.

W.

Lenses for Vision Below Water

IN a communication on the Dioptrics of Vision which appeared in your impression of the 15th December last, I described a form of air-lens for vision beneath the water. Further experience has shown me that the measurements I then gave were not so accurate as they might have been. Thus, the radius of curvature of the glasses in the air-lens to form a lens with a 2 in. focus in water is not $1\frac{1}{4}$ in. as first stated, but 1 in. only.

Again, I somewhat underestimated the magnifying power of the anterior lens of our eye, formed by the aqueous humour, when I set it down as a lens with a focus of 2 inches. $1\frac{1}{2}$ inch is more correct. In accordance with this, I find that for the most perfect vision under water, we require a glass lens of $\frac{3}{8}$ in. focus in air (in place of 1 in. as formerly stated), or an air-lens formed with two segments of a hollow glass globe $1\frac{3}{4}$ in. in diameter, placed concavities outwards. Both these lenses have in water a focus $1\frac{1}{2}$ in. long.

These lenses are for fresh water. Sea water having a greater refractive power than fresh water, requires for perfect vision a somewhat *more convex* glass-lens and a somewhat *less concave* air-lens. I find that an air-lens made with segments of two glass globes of the diameter of 2 inches and $1\frac{3}{4}$ inches respectively, when immersed in sea water forms a lens of $1\frac{1}{2}$ in. focus. But I should observe that good vision under water is obtained by lenses of various magnifying powers, ranging from $1\frac{1}{2}$ to 2 inches focus; but for the distinct vision of small text-type under water, the higher magnifying power is required, and it also is the best for distant vision under water.

53, Montagu Square R. E. DUDGEON

Petrography

THE few English geologists who take an interest in petrography will be thankful to Mr. Geikie for the communications from him which have appeared in NATURE. It is too true, as he observes, that our progress in this branch of geology has for many years been simply *nil*, but there are now manifest signs that this unsatisfactory state of things is drawing to a close. Some of our working geologists are quite aware of the necessity which exists for the application of the microscope to the examination of rocks, and they do not doubt that the result will be proportionately as great as it has been in other branches of inquiry.

It may in the first place be observed that the unsatisfactory nomenclature at present in use is due to the fact that a considerable proportion of the igneous rocks have been named without any precise knowledge of their mineralogical composition, mere chemical analysis being quite inadequate for the solution of the problem; and now there is an evident tendency

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